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SUDDEN DISAPPEARANCE OF AN ABDOMINAL TUMOR.

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IN a late number of the JOURNAL was a case of procidentia of a large portion of the gravid womb of about the fourth month. The chief proof to Mrs. ——'s mind was a similar fact in her first pregnancy. The womb was put in place, and she was told to keep still, in bed, and if up to wear a T strap. I first saw her 1st July, 1859. She called to see me a few days since, and said her child was born July 31, at the fifth or sixth month, was born alive, but soon died. Her health was good.

*Case of Abdominal Tumor which very suddenly disappeared.*

—Mrs. —— gave the following account of her case. Age, 32; for ten years, occupation strictly sedentary, often requiring protracted attention at night; health sensibly impaired; very severe dysmenorrhœa and dysuria; and frequent "bilious attacks," so called. The symptoms of these last were vomiting, purging, and intense colicky pains. The dysuria and colic probably produced by neglected bladder and bowels. Was married two years before I saw her, and was at once relieved from her arduous duties. Has not been pregnant, nor freed from her old complaints. Latterly, has been in constant attendance on a sick member of her new family, which has involved great fatigue and anxiety. Catamenia has continued regular, and at a period a few weeks before I was called, was very profuse, but as painful as ever. The cause of my being called was the discovery of a large, firm tumor in the abdomen, and some new troubles. The principal of these were a very distressing sense of fulness in the abdomen, difficulty in walking, especially up and down stairs, and in rising from a chair, or the bed. To do this, she was obliged to use her arms as levers, her hands firmly seizing and pressing the chair, or bed, and so enabling her to raise herself.

Examination discovered a large, solid tumor, extending from the umbilicus to the symphysis, broadly occupying the correspond-

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ing lateral regions of the abdomen, making the central protrusion less than might have been looked for. The circumference at the highest point was thirty-six inches. *Per vaginam*, the pelvis was more than half filled with a firm, rounded tumor, continuous with that in the abdomen, the os uteri looking toward the sacrum, the cervix being lost in the general intumescence. The sphincter of the vagina was much contracted, and the whole of the vagina, from its beginning and as far as examined, so exquisitely tender as to make the examination painful to the last degree. What was this tumor? An attempt was made at a subsequent visit to introduce the sound, but such was the direction and firmness of the os uteri that I could but just enter it. The tumor was so fixed in the pelvis that it resisted such effort as was made to change its place, and to bring the os within reach. Again, what was this tumor? I have purposely called it abdominal, for though clearly to my mind uterine, I would not give it any distinctive name.

Sept. 3d, 1859.—Treatment—tinct. iodin., to abdomen over the tumor, once daily. Aqua calcis muriat. thrice a day. My compound belladonna ointment to pelvic extension of the tumor, *per vaginam*, once a day.

At the end of a fortnight, tumor, by admeasurement, decidedly diminished. Tenderness of vagina less. Tumor in pelvis and dysuria less. Can rise from chair, and walk, with comparative ease.

At the end of the month, Oct. 3d, tumor no longer felt. Os uteri in place. Cervix natural. Dysuria gone. No complaint.

Mrs. — is a lady of excellent mind and culture. Her occupation made it essential for her to study much, and many things. She could give an accurate account of her feelings and symptoms, and especially of such changes as were occurring in her disease, and of her entire recovery. She was not imaginative, or a fancier of tumors, but in a simple, quiet manner described her case—the relief and the recovery.

In Mrs. —'s case there was not any vaginal discharge, nor increased secretion in any other organ during treatment. Changes occurred very rapidly, but without any other occurrences than have been reported—relief of very distressing symptoms, and, as was said, recovery.

Sir Charles Mansfield Clarke, speaking of the diagnosis of fleshy tubercle of the uterus (or the disease under notice) says:—"It is only in its early stages that it can be mistaken for pregnancy, because when the tumor of pregnancy rises above the brim of the pelvis, the motion of the child may be felt. The tumor of pregnancy after this time increases quickly, that of the fleshy tubercle slowly," &c.

Sir Charles gives the diagnosis of ovarian dropsy, but says nothing of solid ovarian tumors, which may be confounded with fleshy uterine tubercle. How difficult may be the diagnosis of abdo-

minal enlargements is most painfully manifested in the case of Lady Flora Hastings, one of the Maids of Honor of the Queen. Sir Charles, and other distinguished medical men about the Court, pronounced Lady Flora pregnant. She died, and examination showed a large, solid tumor in the cavity of the abdomen—pregnancy never having existed!

Well do I remember a case, in which what was supposed to be an ovarian tumor was removed by excision. The patient died soon after the operation, when it was discovered that the enlarged womb had been cut away, and not an enlarged ovary.

Concerning the treatment of fleshy tubercle, Sir Charles has the following:—"Although no medicine can remove the tumor, there is reason to believe that these tumors have been spontaneously absorbed."—*2d Vol., Eng. Ed., p. 250.*

There can be no question that the removal—spontaneous cure—of this disease, has been effected by absorption. But may not this process be promoted by medicines which are known to increase the action of the absorbents? I have reported cases where there was the best reason in the world for believing this—cases in which tumors have been rapidly growing, but in which, under regular and constant treatment, arrest of growth has occurred, and the tumors have at length entirely disappeared, or which are in a fair way to recovery. These cases are in the JOURNAL. The tumor in the case above reported was clearly and rapidly increasing. Cases of abdominal tumors, having the same character of rapid increase, are in the books; rare, indeed, but in sufficient number for reference. One attained to such size in three months as to fill the abdomen. In Mrs. —'s case, local and constitutional symptoms attended the rapid increase of size. Among these were declining flesh, strength, health, embarrassment on motion, dysuria, and very painful disturbances of the abdominal viscera. After treatment, the abdominal tumor rapidly diminished in size, and the general and local symptoms gave way as this important change proceeded. This was doubtless the result of absorption. At least, there was no evidence of any other agency in the recovery. Did not the treatment aid recovery? I believe it did. "But I do not," says friend —, at my elbow, "I believe they got well by themselves." Sir Charles, and his adherents, get support from my friend. He is an excellent man, of mature age, and deserves to be believed. Let me extend this doctrine a little. Who knows but that all diseases cure themselves? We have just now a distinguished medical authority abroad, and one at home, who, it is said, argue with much force that diseases cure themselves, in spite of the doctor and his pills. To be sure foreign reviewers, I am told, convict these distinguished men out of their own mouths—by their own books—of very false logic, and say that they are not worth the crack of a finger-joint as authorities for their own doctrines. As I have not read their books, I feel under no obligation to read

the reviews which are said to be so overwhelmingly conclusive against them. But does not disease *cure* itself? Let me state a fact or two, which some may think are answers to the question.

CASE I.—Miss —, aged 11, was seized with the formative symptoms of typhoid. The fever soon showed its colors, so to speak, at the mast-head. She was of very feeble constitution—as they say—delicate, small, thin—apparently the very worst of subjects for such an onslaught of such a disease. She was treated after the method of an English physician, who had lived long in a tropical climate, and who by long and careful analysis of the fluids, blood, &c., in fevers, came to the conclusion that a very important change in the amount of saline ingredients of the blood, &c., was a result of febrile action. Upon this hint, he tried non-purgative salts in fever, and was satisfied by results that his theory was right, for the success, at least, of his treatment was great. I had adopted his method in the treatment of fever in the Massachusetts General Hospital for the many years I was attached to that institution, and the result, as the records show, was satisfactory. Miss — was at once put on the use of bicarbonate of soda. Her disease was long—convalescence slow, but perfect. She has been in better health since, than she had ever been before she had fever.

CASE II.—Miss —, a sister of the above, was taken with the same fever. She was *toto cælo* unlike her sister. She was tall, large, of extraordinary development of mind and body, and of fullest health. I have rarely seen a brighter, or a more joyful, healthful being. The typhoid attack was in perfect harmony with her whole state. It was as violent as it could well be. The brain was at once disturbed, and in the first week there was fierce delirium. Two and three persons were in constant attendance to restrain her—to prevent her doing herself great harm. Dr. Jackson saw Miss — with me in the first week. He spoke of the extreme danger of such delirium—he had rarely seen recovery where cerebral disease of this character had occurred in the first week of typhoid. The cases had for the most part been fatal.

Miss — resisted medicine so successfully that she literally took none during her disease. Water she would now and then drink. During convalescence her sister eat freely of calf's-foot jelly. It was her main food. Miss — was a frequent partaker of the same, and grew very fond of it. When it was clear that she must sink from total abstinence, delirium, sleeplessness, &c., some jelly was offered her. She seized upon it as one starved. She eat it constantly, and with a freedom which her morbid—you may say, natural, healthful—instinct demanded. She recovered, almost without the stage of convalescence—lost neither flesh nor color. In six weeks she was about house, and ready for school. Her sister was thirteen weeks before recovery.

CASE III.—Mrs. —, the mother of the above, was seized with



typhoid during Case II.'s recovery. She was entirely "worn out," and had a long, complicated, and dangerous disease. She was prostrated at once, with as little apparent prospect of recovery as any typhoid case within my memory. She recovered, but months elapsed before she regained perfect health.

Being one day in a ward of a hospital, the physician in attendance asked me to look at two patients. They were lying side by side, each on his nice bed, and very closely resembled each other. They were about 25 each, of equal length, flesh, and sound looks. You will rarely meet with two men more nearly alike. "These," said Dr. —, "are cases of acute rheumatism. They came in together, and have made equal progress to recovery. Their treatment has been as unlike as any two things can well be. He has taken colchicum, and liquid farinaceous diet—and he beefsteak, &c., full diet ever since he entered, and you see they are about equally convalescent. I see no difference in this respect between them."

Here are three cases of questionless typhoid, and each of more than average violence. They recovered—two under moderate medication—the saline treatment; one, and apparently the gravest, without a particle of medicine, so called. She recovered in less than half the time of the two others, and without the least apparent waste of flesh—power—health. There are two cases of acute rheumatism, of certainly very striking resemblance in the person, age, and vigor of its subjects—perfect strangers to each other. They were convalescent at the same time, and under most opposite methods of treatment—colchicum in one, beefsteak in the other. Says "the friend at my elbow," "why not then treat all diseases in the same manner—uterine and other abdominal tumors get well of themselves—why not let all diseases alone? I have a young lady with a tumor in the abdomen as large as my fist; why tease her with remedies? You have just quoted Sir Charles M. Clarke, and he and others say that these tumors, when they do disappear, cure themselves. Why—why—but I will not put another question." My "elbow friend" was getting into trouble clearly, for he was getting warm—almost rhetorical—though sinning ordinarily, in this way, much less than any other one of my somewhat extended professional acquaintance.

In answer, I quietly remarked, *hunger* is not one of the symptoms, in the books, of either typhoid, or acute rheumatism. We may lead the horse to the water, but the horse will not always drink. The proverb is somewhat musty, I grant, but its must or age no more hurts it, than does it or they hurt cheese or wine. I think we may err in not obeying instinct oftener. Sir Charles M. Clarke has been quoted on an important and different question indeed; but there was another Sir, whose christian name was John, who always paid profound respect to instinct, and who, if he did not cure disease, certainly saved his life through it, which was quite as important a matter. This fealty to instinct may be some-

times misplaced. Thus the dyspeptic, who eats mince pie for supper, and plum cake at tea, may not sleep as well as if he had disobeyed his instinct. But the sick man may frequently aid treatment. I have just attended two cases of scarlet fever. The first, aged 9, was a very severe case. There were exceedingly painful swellings of the wrists and hands, and of the abdomen; cough, evidently from throat irritation; delirium, sleeplessness, &c. He wanted cider, demanded—cried for it; and this early in the disease. He got cider. His sleep, such as it was, was broken by his sharp, quick cough, which always produced a scream. For this he got Dover's powder. For two succeeding nights he took ten grains each, in divided doses, with great relief. Afterward he got less, until his sleep was good. In short, cider and Dover's powder made up his whole active medication.\* In the second case, aged 11, the disease was less strongly marked; the constitutional and local disturbances being less grave than in the first. He asked for cider; but it was clear that it was rather from *imitation* than instinct; the demand came, and he was not indulged. The swellings were confined to the neck. There was no cough. The skin was hotter, and the pulse quicker. Convalescence began some days earlier than in his brother. These cases are for illustration of a principle, which may declare itself in various ways, each demanding different measures.

There is one view about active treatment which deserves special notice. It may be continued when no longer necessary; especially is this true of *special* treatment. It is a nice matter to know when convalescence begins. We have much about diagnosis, prognosis, &c.; but nobody has systematically treated of convalescence. The doctrine of crises necessarily involved the nature, signs and treatment of convalescence; and he who could see a crisis coming, or knew it when it had come, had the very best guidance for the after treatment of disease, or rather of the stage of convalescence. Hippocrates did not actively interfere with the proper symptoms of disease. All he attempted was to palliate them. When a crisis came, its treatment depended on its perfect or imperfect development. We have lost sight of much of this old method of inquest and treatment. We have, some of us, some hints about the length of disease, or when change may be looked for. Thus we may look for change about the eighth day of pneu-

\* The opiate treatment of inflammatory diseases, has been steadily gaining ground. Dr. Sam'l Danforth, I think an early President of the Massachusetts Medical Society, and Dr. Fisher, of Beverly, a President of the same, were both of them strenuous advocates of opium in inflammation. In later times, we have Prof. Alonzo Clark as its advocate, in the treatment of puerperal fever, occurring, too, in hospitals, where it is always so fatal—as Wm. Hunter says, always is so. Prof. Clark has used opium with unequalled success, and in quantities almost fabulous. Dr. John Ware bears witness to its usefulness in croup. Stewart found it, in large doses, highly efficacious in uterine hemorrhages after labor. But it is not necessary to multiply authorities. Opium is used for removing pain. Has it not a higher office? Does it not alter that condition on which pain depends? Pain is sensation occurring in structures which are not the natural organs of sense, and is produced by a morbid state of such structures. The office of opium is either to alter such condition, or to prevent its communication to the brain. It thus either removes morbid conditions, or places them in a state either for self cure, or for the best action of remedies.

monia; and the fifth day of puerperal peritonitis has been thought to present signs or facts of change, or crisis. In cases of those diseases, which may terminate favorably, some remission of symptoms or rather signs of recovery may be observed. A wider observation, or a closer attention to these circumstances in disease, might essentially aid in answering those most difficult and important questions concerning further active medication—namely, how much longer may such treatment be safely continued? and when convalescence may be known to have begun?

If I am now asked what I would do in regard to abdominal tumors, of which an instance is in this paper, my answer is, that I would do what would best promise to promote their absorption, or disappearance, as I have already done, and after the use of which, these tumors have, in more than one instance, disappeared, and in others have been checked in their growth. No matter what may be the size of such tumors—whether of a fist or a bushel measure, I would labor faithfully to promote their removal, and by means which would not disturb existing general health. I believe we have such means. They have been tried, and my purpose is, as opportunity occurs, to try them again.

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#### BRONCHOPHONY—LAENNEC AND SKODA.

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AMONG the doctrines which Skoda holds in opposition to Laennec, his explanation of bronchophony by the theory of consonance holds, as is well known, a prominent place. Markham, the translator of Skoda's work, remarks in his preface, that "whatever may be the fate of this theory, one thing may be fairly predicted to result from Skoda's enunciation of it, and from a consideration of the experiments and reasonings on which he bases it, and that is, a recognition of the fact that Laennec's explanation of bronchophony by the increased sound-conducting power of consolidated pulmonary tissue, is not true, at least in every case." It must be true or false in every case, nevertheless, in principle. A recent writer\* on auscultation of the voice, expresses his views in reference to Skoda's "experiments and reasonings," in disproof of Laennec's explanation of bronchophony, as follows: "Skoda appeals in the most unbiassed manner to a few well-established principles of natural philosophy, and to a few quite plain experiments instituted by him, to prove beyond a doubt the utter fallacy of the theory of Laennec, which was adopted throughout France; and it is astonishing how, nearly twenty years since Skoda first opened publicly his contest against Laennec, with weapons no other than sound logic and an unbiassed mind, there should still to this day, here and there, *cis et trans mare*, exist some stragglers of the

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\* Dr. J. Henske. N. Y. Journal of Medicine, 1859.

French army. But every cause, every idea, and every theory, finds its Epigones—be it table-moving, know-nothingism, or some special Franco-mania."

Skoda's admitted disproof of Laennec's views upon this subject appears to have given rise to a series of fruitless attempts to supply the supposed deficiency in auscultatory science, and thus to secure a prize that eluded the grasp of Laennec himself. Have we, in these repeated failures, an illustration of the principle that "no superstructure can be secure while its basis rests on untruth"? Or was Laennec at fault in his "principles"? The ground will be taken, in the present communication, that Skoda furnishes no proof whatever of the "fallacy of Laennec's theory," notwithstanding the alternative presented the writer of being ranked among the "stragglers," "table-movers," &c. And as the asserted disproof of Laennec turns mainly upon the "established principles," it will be unnecessary to regard the experiments of Skoda, which have also been contradicted in their results by others.

Now the gist of Skoda's argument consists in the proof he is supposed to have adduced, that air is superior to solid bodies in sound-conducting power, contrary to "the generally-received opinion" and that of Laennec, and consequently that healthy lung parenchyma is a better conductor of sound than consolidated lung tissue. The proof that air is the better conductor of sound, is comprised in the following data, or "principles." "The human voice, and every other sound which is formed and propagated in the air, is heard farthest in the air." "A sound in one room passes with difficulty into another." "Any one wishing to weaken his hearing, stops his ears." The ticking of a watch is heard more distinctly through a hollow tube than through a solid cylinder. "It is a remarkable circumstance," Skoda further observes, "that auscultators should make use of a hollow tube, and not solid cylinders, and yet assert that dense bodies are better conductors of sound than air."

It is apparently out of deference to Skoda's statement that "the human voice is heard farthest in the air," that Walsh concedes the superior conducting power of air. "In regard of conduction," he says, "theory would say that as the human voice is best propagated in air, the more the lungs are rarefied, the higher would their conducting power become," &c.

But is it not true, also, that sound formed in a given solid is heard farthest in the solid? "The slightest scratching at one end of a long rod may be heard," says Skoda, "if the ear be brought in contact with the other end; while no sound whatever is audible in the air, although the ear be brought much nearer to that end of the rod whence the sound proceeds." That sound formed in water is heard farthest also in the water, Skoda furnishes the following proof. "The sound formed by striking two stones together, under water, is distinctly heard there, and even causes a disagreeable

sensation, while out of the water it can be scarcely recognized." These facts furnish the following statement: sound may be heard in the medium in which it originated, while other media in contact are not sensibly affected at an equal distance, or "sound is heard farthest in the medium in which it is originated." But does the fact that sound formed in the air is heard farthest in the air, that sound formed in a solid is heard farthest in the solid, and that sound formed in water is heard farthest in the water, prove that air is the best conductor? If so, it proves, at the same time, the absurdity that each of the media concerned is the best conductor. It therefore proves nothing as to their relative conducting power. That constitutes an entirely new and distinct question, which, though professing and admitting to have settled, Skoda does not even touch. It appears to be "well established" in acoustics, however, that while sound may be generated and propagated in all elastic media, its velocity and intensity are, other things being equal, directly as the densities of the media it traverses.

But the facts embraced in the above quotations, according to Skoda, not only prove the superior sound-conducting power of air, but also "show that sound does not pass readily from dense bodies into the air, or from the air into dense bodies." Now if we understand sound in elastic bodies to mean simply elastic bodies in a state of vibration, the above generalization will admit of the following change of phraseology: dense bodies in a state of sonorous vibration do not readily excite sound in the air, nor do the sonorous vibrations of the air readily generate corresponding vibrations in dense bodies. But do not the vibrations of the bell, the drum, the violin-string, &c., readily excite sound in the air? So readily, in fact, does sound pass from solid bodies into the air, contrary to Skoda, or so easily is the air thrown into sonorous vibrations by the vibrations of solid bodies, that one solid can scarce impinge upon another, without producing sound in the air of some sort. Indeed, the vibrations of solid bodies, excited by their mechanical action upon each other, constitute the common (though not universal) source of sound in the air. On the other hand, it appears to be true that sonorous vibrations in the air have but a feeble effect upon solid bodies, or "sound does not pass readily from the air into dense bodies." The vibrations of the bell, excited by the stroke of the tongue, readily excite sonorous vibrations in the air, but the vibrations or sound of the bell in the air, by impinging upon a second bell in immediate proximity with it, may have no perceptible effect upon it. The mechanical action of solids upon each is requisite to the more full development of their sonorous elasticities. The relations of air to solids and of solids to the air, when in a state of sonorous vibration, thus appear to be widely different, though Skoda would have us understand that sound passes with equal difficulty from the air into solids, and from solids into the air. The reason, too, is readily apprehended when we

consider that the momenta of sonorous waves are, other things being equal, like their velocities, or the densities of the media in which they reside, and that it is by the mechanical impulse of these alone that an original sound is re-produced in a new medium.

Nevertheless, the vibrations of solid bodies must have a certain degree of intensity to excite appreciable sound in the air. The vibrations of the rod, generated by slightly scratching one end (a common illustration of the ready propagation of sound through solid bodies, when once originated in them, and instanced by Skoda, ostensibly for the purpose of receiving an explanation in conformity with the views he is endeavoring to establish, which is the vague statement that "no part of the sound passes off from the rod into the surrounding air, but remains wholly concentrated in it") may be too feeble to excite appreciable sound in the air, though they are readily propagated, or "conducted," throughout the rod.

If "a sound in one room passes with difficulty into another," it is evidently because the aerial waves are too feeble in their mechanical impulse to reproduce the sound in the solid walls of the room, not because sound may not be as readily propagated in the walls of the room as in the air, when once established in them. The auscultator uses a hollow tube for the reason that the sound passes from the surface of the chest to the ear more perfectly through one medium, the air in the tube, than through two media, the solid cylinder and column of air between it and the tympanum of the auscultator. Also because the vibrations of the surface of the chest have a greater effect upon the air, than upon the solid cylinder; that is to say, the sound is more easily and perfectly reproduced in the air than in the solid cylinder, air being peculiarly susceptible of sonorous vibrations from solids, on account of its relative density and specific elasticity. The establishment of sound in the air and cylinder is, however, a very different thing from the propagation, or conduction of the sound, after being generated in them. The sound will pass the rod with the greatest rapidity and intensity, though greater mechanical force is requisite for its generation in the rod than in the air.

In short, every fact adduced by Skoda in proof of the superior conducting power of air, relates simply to the general proposition, that sound is heard farthest in the medium in which it is originated; or, as a corollary to it, that sound passes more perfectly from one given point to another through a single medium than through two media. Nor do "the experiments and reasonings" of Skoda determine anything as to the relative conducting power of different media. It may be well to have the confirmation of this statement from Skoda himself. "The difference in the conducting power of air, wood, and other bodies," says he, "has not been experimentally determined." Nothing, then, most certainly has been determined against Laennec's views upon this subject.



# REMARKS ON ANÆSTHESIA, AND THE AGENTS EMPLOYED TO PRODUCE IT.

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[From the British and Foreign Med.-Chir. Review.]

THE discovery by which surgical operations can be rendered painless is one of the greatest connected with our profession, second only to that of vaccination. It is a blessing to the human family that cannot be overrated; and having been among the first to make a successful use of it in surgical practice, I thought that a brief sketch of the history of anæsthesia, and some remarks on the comparative value of the agents employed to produce it, would not prove uninteresting.

It was my fortune to perform the first capital operation on a patient rendered insensible by the inhalation of sulphuric ether. This was done on November 7th, 1846, at the Massachusetts General Hospital, Boston. On September 30th, preceding, Dr. Morton, a dentist, administered it to a man, from whom he extracted a tooth without causing pain. Almost immediately after, he requested the late Dr. John C. Warren, who was at that time the acting surgeon at the hospital, to use it at that institution. Dr. Warren consented. It was inhaled by a patient, with partial success, on whom Dr. Warren operated on October 16th. The operation was the removal of a nævus from the face. On the day following, I extirpated a large fatty tumor from the arm of a female, who was made wholly unconscious and insensible by the inhalation of the ether. The operation lasted seven minutes.

At that time Dr. Morton was, I thought, the only person who knew what the anæsthetic agent was. On November 1st, I took charge of the surgical department of the hospital, and in a day or two after Dr. Morton asked me if I were willing to allow him to administer his "composition," as he called it, to a female whose limb I was about to remove above the knee. I told him I would not, unless I knew what the article was, and felt confident of the entire safety of its administration. He at once told me that it was rectified sulphuric ether. He allowed me to communicate this to my colleagues, with an understanding that it should not be made known publicly, until he had obtained a patent, for which he had already applied. On the following day the operation was performed, in the presence of more than two hundred spectators.

It rarely falls to the lot of a professional man to be the witness of a scene of more intense interest. The operating room was crowded. Many were obliged to stand. Besides the class of students in attendance on the lectures, numbering more than a hundred, and many of the principal physicians and surgeons of the city and neighborhood, there were present several clergymen, lawyers, and other individuals from the various callings of life. When I entered the theatre, before the patient was brought in, I found

it, to my surprise, filled in every part, except the floor on which the table stood, with persons on whose countenances was depicted the almost painful anxiety with which they awaited the result of the experiment they were about to witness. I simply told them that I had decided, with the advice of my colleagues, to allow the patient, on whom I was to operate, to inhale an article which was said to have the power of annulling pain. The patient was then brought in. She was a delicate-looking girl of about 20 years of age, who had suffered for a long time from a scrofulous disease of the knee-joint. It had at length suppurated; there were extensive openings into the cavity of the joint; the cartilages were ulcerated and partly absorbed; the bones carious, and symptoms of hectic fever had already made their appearance. As soon as she was well arranged on the table, I told her that I should let her breathe something which, I hoped, would prevent her from suffering much from the operation, and that she need not be afraid of breathing it freely.

As the ether was at the time administered by means of a large and clumsy instrument, which required to some extent the coöperation of the patient, it was desirable that the amputation should be done as rapidly as possible. Everything, therefore, was arranged with this view. I decided to perform the flap operation. One person was to compress the artery, another to withdraw the flaps, a third to hand the instruments, and a fourth to watch the pulse. I grasped the patient's limb with my left hand, and held the amputating knife behind me in my right, carefully concealed from her view. The mouthpiece of the inhaling instrument was then put into her mouth, and she was directed to take long inspirations. After breathing in this way a short time, the nostrils were compressed, so that all the air that went into the lungs must first pass through the machine, and of course be mixed with the vapor of the ether. She breathed with perfect ease and without struggling, and in about three minutes from the time the instrument was put into her mouth, Dr. Morton said, "She is ready." A death-like silence reigned in the room; no one moved or hardly breathed. I passed the knife directly through the limb, and brought it out as rapidly as I could, and made the upper flap. The patient gave no sign of feeling or consciousness, but looked like one in a deep, quiet sleep. Every other person in the room took a full inspiration that was distinctly audible, and seemed to feel that they could now breathe again. The second flap was then made, the bone sawed, five arteries were tied, and as I was tightening the ligature upon the sixth and last, she groaned, being the first indication of sensibility that had been given. Nothing more was done than to bring the flaps together, cover the stump with cloths dipped in cold water, and apply two or three turns of a roller to keep them in place. Her consciousness soon returned; she was wholly ignorant that the operation had been done. For some

time she would not believe it, and said that she had felt nothing till I tied the last artery. The operation lasted a minute and three quarters, not including the time required to tie the arteries. I did it rapidly, though it has been done in less time, because I feared that the insensibility might pass off, and we had no means then, as we have now, of continuing it as long as is necessary.

Patients who have inhaled ether, when its effects are at first passing off, are usually bewildered, not easily controlled, and by no means inclined to do as they are desired. It would be almost impossible to persuade one of them at such a time to breathe through the instrument that was then in use. At present, fortunately, we can keep up the state of anæsthesia as long as we wish, by administering the agent employed for this purpose by means of a sponge. This simple contrivance was first used at the Massachusetts Hospital.

The patient whose case I have just spoken of recovered rapidly from the operation, was in good health when I left home eleven years after, and I have no reason to suppose that she is not so at the present time.

It will be readily believed that a result so successful, and witnessed by so many intelligent persons, made it impossible to doubt the anæsthetic power of the agent employed, and what this was very soon became known. In an almost incredibly short space of time, numerous operations were performed on persons rendered insensible by the inhalation of ether, in various parts of the United States and Europe, and there is hardly a country in Christendom in which it has not been thus used to a greater or less extent.

*The Anæsthetic Agents.*—These are sulphuric ether, chloroform, chloric ether, and amylene. The two latter are now rarely used for this purpose, and probably never will be again. Chloric ether is simply a tincture of chloroform. There are two kinds, one the concentrated and the other the chloric ether of commerce. The first is composed of one part of chloroform to nine of alcohol; and in the other there is one part of chloroform to fifteen of alcohol. It can be prepared by mixing the two ingredients of which it is composed in the proper proportions, and if the alcohol which it contains be evaporated, nothing but chloroform remains. It is evident that it derives its anæsthetic properties from the chloroform, and it is therefore as unsafe as that article; for the alcohol, though it renders it less efficacious, does not make it more harmless.

*Amylene*, the chemical elements of which are equal parts of carbon and hydrogen, has caused death in several instances. There have been so many fatal cases in proportion to the number in which it has been exhibited, that no one hereafter will probably be sufficiently reckless to use it.

*Chloroform* was first employed by Professor Simpson, of Edinburgh, who thought that it possessed "various important advantages" over sulphuric ether. He said that it was more portable,

more agreeable to inhale, less exciting, and that it gave a greater control over the patient. That it is more portable and more agreeable to inhale, I admit, but that it is less exciting and a more efficient anæsthetic agent, I deny. But the principal objection to it is, that its inhalation sometimes causes death. Its advocates admit that this has occurred in sixty cases, while others believe that there has been double this number. But be the number what it may, so many have died from its inhalation, that many persons are in favor of abandoning its use altogether. Death produced by it cannot now be attributed in every instance, as it was at first, to the impurity of the article, or to the exhibition of too large an amount, or to the want of skill or judgment in the administrator. There have been several fatal cases lately, where the chloroform was said to be of the purest character, and a small quantity only inhaled, and this, too, in the presence and under the direction of intelligent, well-educated and careful men.

The truth is, that chloroform, when inhaled, acts on the system in a way that is not yet well understood, and may destroy life in spite of the utmost caution. Its effects are sometimes so sudden, that no foresight can prevent a fatal result. Unless some means, therefore, can be discovered that will render its inhalation safe, common prudence and a regard for human life would seem to dictate that it should be no longer used in this way. It is true that the state of unconscious insensibility produced by it is a blessing of countless value to those who are to undergo severe surgical operations, not only by rendering them painless, but at the same time disarming them of their terror. And these are not the only advantages of anæsthesia. It in great measure prevents the shock to the nervous system which not unfrequently defeats the skill of the most expert surgeon, it enables him to operate more deliberately, removes all necessity for haste, which is often the result of the sufferings of the patient, and makes the performance of some operations comparatively easy, which in the ordinary state of the system could hardly be done at all. It is not, therefore, to be wondered at that professional men are reluctant to abandon the use of chloroform, and their unwillingness might be excused if there were not a substitute equally efficacious, as easily administered, and entirely safe. That rectified sulphuric ether is such a one, I have no doubt. I have witnessed its effects on several hundred patients upon whom severe surgical operations were performed, and all of them were rendered motionless, unconscious and insensible. In no instance was there any alarming or serious consequence. It does not act as speedily, perhaps, as chloroform, but in no case were more than eight minutes required to produce complete anæsthesia. It can be effected in much less time when atmospheric air is not allowed to mix freely with the vapor of the ether. This is the method pursued in the hospital at Naples, where no other anæsthetic agent is used; and I saw a patient undergo a

severe surgical operation there without the slightest suffering, who was brought into this state by inhaling ether only a minute and a third! But when administered thus rapidly, it is apt to produce a distressing cough and sense of suffocation for a moment, and there might be some reason to fear asphyxia from the exclusion to too great an extent of atmospheric air. Professor Polasciano, however, told me that he always gave it in this way, and had never seen any more troublesome symptoms than those I had witnessed in the case just alluded to. These, though distressing to the patient, were of short continuance, and by no means alarming.

There is no doubt in my mind that sulphuric ether should be used as an anæsthetic agent to the entire exclusion of chloroform. It is as efficacious, and I should say without hesitation, after having seen chloroform administered by others in many cases, that ether produces a more complete state of unconscious insensibility. Its effects pass off sooner, and less vomiting, nausea and headache follow its inhalation. It is as easily administered. All that is required for its administration is a bell-shaped sponge, with a concavity large enough to cover the nose and mouth. If the patient breathes it gradually, little or no irritation is produced in the larynx and air-passages, there is but little if any cough or sense of suffocation, nor a distressing or unpleasant symptom of any kind.

There may be some persons to whom the odor of ether is offensive and irritating, but they are comparatively few, and even they can be brought under its influence without any very great annoyance.

The quantity of sulphuric ether required to produce anæsthesia depends very much on the manner in which it is administered. If the patient is made to inhale it rapidly, and the atmospheric air is to a great extent excluded, a small amount will be sufficient. From four to eight ounces may be regarded as the average quantity. It is rare to meet with a case in which less than four ounces will be used; and in protracted operations, in which it is desirable to keep up the state of insensibility for a length of time, I have often given more than eight ounces. The ether should at first be poured on the concave part of the sponge; one or two ounces will be enough for this purpose. When the inhalation is going on, it is better to pour the ether on the outside of the sponge, so as to avoid the necessity of removing it from the face. From half an ounce to an ounce should be used at a time in this way, till anæsthesia is produced. When this takes place, the patient is wholly unconscious, and has no control over the voluntary muscles. He is unable to raise his eyelids when told to do so, and gives no indication of hearing or consciousness, if spoken to in a loud tone. The pulse usually becomes slower than the ordinary standard, though at the beginning of the inhalation it is quicker.

It is, I am confident, a perfectly safe anæsthetic agent. I have not been able to find any well-attested case of death from its in-

halation. There may have been such, but they have never come to my knowledge, though I have taken unwearied pains to obtain information on this point.

It has been said, that this may be attributed to the fact that ether is not extensively used, but that if it were, there would probably have been as many fatal cases in proportion from it, as from the inhalation of chloroform. But this statement is not strictly correct; for though ether is not employed as an anæsthetic agent to any extent, if at all, in Great Britain or many parts of Europe, it is used in Lyons, Naples, and is almost the only one that is administered in the principal hospitals of the United States of America, where its now familiar properties were first discovered.

I have given it in several hundred cases, and witnessed its exhibition by others in as many more. I have administered it to infants not three weeks old, and to persons more than threescore years and ten, and have never in a single instance seen an alarming or distressing effect produced by it. On the first introduction of ether into surgical practice, it was not thought safe to allow persons to inhale it in whom there was reason to believe there was any disease of the heart or lungs, or who had any tendency to an affection of the brain and nervous system. But for some years past I have been in the habit of administering it to individuals of this description, and have as yet had no cause to regret it. In such cases I have thought it prudent to have the vapor of the ether inhaled more slowly, so that it may be more diluted with atmospheric air than under ordinary circumstances; of course the patient could not be brought as soon under its influence as when taken in the usual way.

The state of the system which is produced by the inhalation of ether is that of narcotism, similar precisely to what is induced by drinking immoderately wine or other alcoholic liquors. It is a state of intoxication more transient and less dangerous than that from alcohol. Its effects pass off sooner, because the vapor of the ether begins to escape from the lungs as soon as the patient ceases to inhale it; while alcohol taken into the stomach is carried into the circulation, and mixes with the blood, and in this way acts longer, if not more powerfully on the brain, though its narcotic effect is not so soon produced. It is possible that life might be destroyed by the inhalation of ether, if it be continued uninterruptedly for a great length of time and a great quantity inhaled. Fatal congestion of the brain might thus be produced, as sometimes happens when alcoholic liquor has been taken to excess. But no person of ordinary prudence would administer it in this way. Long before the occurrence of such a result, symptoms of an unequivocal character would indicate the approaching danger.

When death follows the inhalation of chloroform, on the other hand, there is no merciful premonition. The late Dr. Snow, whose



experience on the subject was perhaps greater than that of any other person, thought that "*sudden palsy of the heart* is the cause of sudden death from chloroform." In death by asphyxia, the heart beats for some minutes after breathing has ceased; "whereas in some cases of death by chloroform, the breathing has been proved to go on up to the time the pulse stopped, and after it."

With the hope that those who may have occasion to employ any anæsthetic agent will at least make a fair trial of *rectified sulphuric ether*, I respectfully submit these remarks to my professional brethren.

### Reports of Medical Societies.

EXTRACTS FROM THE RECORDS OF THE BOSTON SOCIETY FOR MEDICAL IMPROVEMENT. BY F. E. OLIVER, M.D., SECRETARY.

OCT. 10th.—*Case of Abdominal Tumors Simulating Pregnancy.* Dr. STORER reported the case.

Mrs. H., 36 years of age, had one child five years since. Until April last, she enjoyed good health, and was perfectly regular in her menstrual periods, both as to length of continuance, and quantity. At that time, she ceased to menstruate; her abdomen soon after began to enlarge in the region of the uterus; she complained of nausea, and she and her family supposed pregnancy to exist. The abdomen gradually enlarged, until she suffered so much from dyspnœa as to cause her great distress, and to compel her to consult her family physician, who also considered her *enceinte*.

I visited her on the 25th ult. She was in bed, lying on her back, with her knees elevated. She seemed much emaciated; her countenance was anxious, from constant distressing dyspnœa. The abdomen was much enlarged—more so than is commonly the case at the fifth month—presenting at the lower portion the usual appearance of pregnancy, in its form, with an unusual quantity of fluid, the intestines being crowded and entirely above the umbilicus.

On the right side of the *linea alba*, a firm, resisting body could be distinctly felt, which resembled the head of a fœtus. This, upon pressure being made, readily receded. Opposite this, on the left side of the *linea alba*, was also perceived a resisting body which seemed to be the extremity of the trunk, which was similarly affected by pressure. Scarcely any change was perceptible in the cervix uteri. Upon the patient's assuming the erect posture, ballottement was produced, as perfectly as I ever felt it. As in every case where I had felt ballottement the woman was found to be pregnant, and as I was acquainted with no writer who had met with this characteristic sign except at this period, I concluded that pregnancy existed, and decided to produce premature delivery.

At my next visit, on the 26th ult., assisted by my friend, Dr. Borland, who visited her with me daily, and coincided with my views of the case, I injected a quantity of tepid water into the os, that a separation of the membranes might be produced from the uterus, without a loss of the liquor amnii. As no uterine contractions had commenced on the 28th, I passed the uterine sound, and found the organ empty.

I now told the family that a solid body was in the cavity of the abdomen; what it was, I could not determine, but I thought it must be a case of extra-uterine foetation; and if so, it ought to be removed, as in the patient's exhausted and sinking condition she could survive but a few days. Dr. J. Mason Warren saw her the next day, and after a careful examination of her case, agreed perfectly with my diagnosis and proposed treatment.

Unavoidable circumstances prevented me from operating until the 3d inst. At this time, assisted by Drs. Warren and Borland, the patient being fully etherized, I made an incision through the linea alba, below the umbilicus, from three to four inches in extent. Upon making a small opening through the peritoneum, a large quantity of fluid gushed out. Passing the hand into the abdominal cavity, an oblong body was felt upon the right side, resembling a substance enclosed in a cyst. This was carefully slipped through the aperture, and proved to be a diseased ovary. A strong ligature being applied to the broad ligament, the diseased mass was removed.

A second, and larger tumor, was now found occupying the left side of the abdomen, which upon its removal was also found to be an ovary: it was treated as the former.

The operation was performed with great ease. No difficulty was experienced from the protrusion of any portion of intestine through the incision. Scarcely an ounce of blood was lost, and I cherished the hope that my patient might do well. During the succeeding thirty-six hours, she was very comfortable, and expressed much gratification that the operation had been performed. At the expiration of this time, peritonitis supervened, and she died on the third day.

*Remarks.*—The experience of every physician must have taught him the utter impossibility, not unfrequently, of diagnosing abdominal tumors. It is unnecessary to refer to individual cases where mistakes have occurred; with some, you are all familiar.

Several of the circumstances in the case just reported, were so peculiar as to leave but little doubt that pregnancy existed. Previous to April, the catamenia had been perfectly regular. Upon their cessation, peculiar sensations were experienced, such as slight nausea, more or less pain in the back, and general uneasiness. In a short time the abdomen began to change in appearance. At first, a gradual enlargement took place, which became more rapid during the last month of the patient's life. The peculiar feel of the resisting body through the abdominal parietes, and the perfect ballottement, seemed, previous to the passage of the sound, to indicate *intra-uterine* pregnancy; and when this proved not to be the case, *extra-uterine foetation* appeared to be the most probable condition.

The case proves incontestably that ballottement, as perfect as in pregnancy, may exist when the uterus is empty and a solid body floats freely in ascites.

*Description of the Tumors by Dr. Ellis.*—The growths had a flattened oval form. The largest was eight inches long, five wide and three thick. Externally, it was lobulated and vascular. The cut edge of the band divided in the removal of the mass, was two inches and a half long and a quarter of an inch wide. At one extremity of this, a portion of the growth, two or three inches in diameter, was of a yellowish color and presented the appearance of some mammary glandular formations. The cut surface had a fibroid character and was in

some parts vascular. The tissue, though firm, was every where infiltrated with serum, and at one part there was a well-marked cyst, about half an inch in diameter, with a secondary cyst projecting into it. Many round, firm granulations, from one to two lines in diameter, were sparsely disseminated over the surface. A number of round, reddish nodules, from a quarter to half an inch in diameter, were also seen.

The smaller mass was five inches and a half long, and four broad. It resembled the other, with the exception that it was somewhat paler, and near the external surface presented a peculiar radiated appearance, as from the separation of the fibres by serum. In one part a little pus was seen.

Examined with the microscope, the greater part of the growth was found to be fibrous. In the small granulations were a few small, indistinct nuclei. In the yellowish lobular portion were lobules filled with large granular corpuscles of various sizes, without nuclei or nucleoli. There were also many free corpuscles of the same character, and much fat.

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## THE BOSTON MEDICAL AND SURGICAL JOURNAL.

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BOSTON, NOVEMBER 17, 1859.

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APPLICATION FOR THE PRIZE OF M. BREANT.—Some years ago, M. Bréant, in France, left the sum of £4,000, to be given to the discoverer of a specific for the treatment of cholera. The conditions are that the remedy should, in the opinion of the Imperial Academy of Medicine, be as undoubted in its effects as is quinine in the cure of intermittent fever. Lately a Dr. Pickering, of York, England, has urged his claim for the reward, but on account of the irregular manner in which his remedy was presented to the Academy, it was not favorably received by that body. Dr. P. then made a personal appeal to the Emperor, and succeeded in enlisting the sympathy of that august personage. A communication was sent to the Academy, by the Minister of Public Instruction, together with Dr. Pickering's letter, and a request to be informed what steps had been taken by the Academy in the matter. It was stated in reply that Dr. Pickering had announced his discovery to the Academy, and had forwarded samples of his medicines, but refused to divulge the formule without a pecuniary compensation. The Academy had consequently no alternative but to reject the application, and to exclude him from the *concours* for the Bréant prize. Dr. Joseph Ayer, of London, was also an unsuccessful applicant for the prize. His remedy consisted in small doses of calomel and laudanum, repeated every five or ten minutes during the period of collapse.

THE CLIMATE OF AUSTRALIA.—We translate the following passage from a recent work by M. Thierceelin, on the Treatment and Curability of Pulmonary Consumption:—"Everybody knows that England is the country of predilection for phthisis; that this disease is especially prevalent and fatal in the cities of Great Britain, and that there, as everywhere else, it attacks chiefly the lowest ranks of society. It is also well known that Australia has been peopled, in great part, by the class most predisposed to phthisis—by paupers and malefactors. Now I spent six months at Sidney, the capital of Australia, where I looked for consumptives, but found hardly any. Everywhere the people were vigorous and glowing with health. Moreover, mothers who had lost phthisical and scrofulous children in England, had reared up in the new country numerous families in luxuriant health.

What had hereditary predisposition done here? Nothing; its influence had disappeared with the circumstances predisposing to the disease.

**IMPORTANT LIBEL CASE.**—The suit brought by Dr. Ira Barrows against Dr. D. H. Storer, of this city, which has, in a different shape, been before the Courts for some years, was finally brought to trial in New Bedford last week, and resulted in a disagreement by the jury. The alleged libel was first published in this JOURNAL, and Dr. L. V. Bell was the original defendant in the case.

**TREATMENT OF DYSPEPSIA.** *Messrs. Editors.*—In the course of a long practice in cases of indigestion, I have found the following prescription to have done good service. R. Prep. carb. iron, calc. magnesia, pulv. elm bark, each  $\mathfrak{z}\text{i}$ .; pulv. cubebæ,  $\mathfrak{z}\text{ss}$ . M. Take a teaspoonful, half an hour before eating, in half a tea-cupful of water.

A PRACTITIONER.

**DR. HAYWARD ON ANÆSTHESIA.**—We re-print from the last number of the *British and Foreign Medico-Chirurgical Review* an able paper by Dr. GEO. HAYWARD, on Anæsthesia, in which, of course, preference is given to rectified sulphuric ether, as being equally efficacious and infinitely more safe than chloroform. We trust it will awaken the attention of the profession in England to the dangers of the latter agent, and to the advantages of ether, which has not yet been proved to have caused a single death.

**HEALTH OF THE CITY.**—Smallpox continues to be the chief fatal disease next to consumption; of the 7 victims to it, 4 were adults, and 3 were children from 5 months to 7 years. We notice 4 deaths from croup and 2 from pneumonia. There were but 22 deaths of children under 5 years of age, and 31 of subjects between 20 and 60. Of the 10 deaths from consumption, 6 were of females and 4 of males. The total number of deaths for the corresponding week of 1858 was 75, of which 16 were from consumption, 4 from pneumonia, 0 from smallpox, and 1 from croup.

**DISINFECTANTS IN PARIS.**—Ever since Messrs. Corme and Demeaux proposed sulphate of lime and coal tar as a disinfectant, purifying agents have been the order of the day. The merit of the discovery was of course at first disputed; and every one who thought he could contrive some disinfecting compound sent papers and samples to the Academy of Medicine or of Sciences. The last applicant is M. Boinet, well known by his works on Iodine, who, in a paper read Sept. 20th, before the Academy of Medicine of Paris, contends that the foulest sores can be rendered perfectly sweet by applications of tincture of iodine. There will be no harm in trying this agent, which, no doubt, has already rendered very great service.—*London Lancet*, October 8th.

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*Books and Pamphlets Received.*—Proceedings and Debates of the Third National Quarantine and Sanitary Convention. (From Dr. John H. Griscom.)—Annual Address delivered before the Connecticut Medical Society. By Benjamin Hopkins Catlin, M.D., President of the Society.—Illustrations to How to Work the Microscope. By Lionel Beale, M.B., F.R.S. (From the Author.)

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**MARRIED.**—At San Francisco, Cal., Oct. 6th, Dr. Hugh H. Toland to Mrs. Mary B. M. Gridley, daughter of the late Dr. Morrison, of Dresden, Me.

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**DIED.**—At New York, 7th inst., Guy Carleton Bayley, M.D., 74.—At Beverly, N. J., 5th inst., John M. Brewer, M.D., formerly of Philadelphia, and a native of Framingham, Mass.—In this city, 12th inst., Mrs. Harriet Morland, widow of the late Robert Morland, Esq., and mother of Dr. W. W. Morland, aged 67.

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**Deaths in Boston** for the week ending Saturday noon, November 12th, 68. Males, 32.—Females, 36.—Apoplexy, 1.—asthma, 1.—inflammation of the bowels, 2.—inflammation of the brain, 1.—congestion of the brain, 1.—cancer (in the stomach), 1.—consumption, 10.—convulsions, 1.—cholera infantum, 1.—croup, 4.—diarrhœa, 2.—dropsy, 2.—dropsy in the head, 2.—dyspepsia, 1.—debility, 1.—infantile diseases, 3.—puerperal diseases, 2.—erysipelas, 1.—typhoid fever, 3.—disease of the heart, 3.—inflammation of the knee, 1.—laryngitis, 1.—congestion of the lungs, 1.—inflammation of the lungs, 2.—marasmus, 1.—old age, 1.—palsy, 2.—pleurisy, 1.—peritonitis, 1.—smallpox, 7.—whooping cough, 1.—unknown, 2.

Under 5 years, 22.—between 5 and 20 years, 8.—between 20 and 40 years, 17.—between 40 and 60 years, 11.—above 60 years, 7. Born in the United States, 42.—Ireland, 19.—other places, 7.